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DOES TEMPORARY AFFIRMATIVE ACTION PRODUCE PERSISTENT EFFECTS?

A STUDY OF BLACK AND FEMALE EMPLOYMENT IN LAW ENFORCEMENT[†]

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ABSTRACT

This paper exploits variation in the timing and outcomes of employment discrimination lawsuits against US law enforcement agencies to estimate the cumulative and persistent employment effects of *temporary* externally-imposed affirmative action (AA). We find AA increased black employment at all ranks by between 4.5 and 6.2 percentage points, relative to national trends. We also find no erosion of these employment gains in the fifteen years following AA termination, although black employment growth was significantly lower in departments *after* AA ended than in departments whose plans continued. For women, in contrast, we only find marginal employment gains at lower ranks.

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1. INTRODUCTION

In the decades following the passage of the 1964 Civil Rights Act, state and local police agencies across the country were sued for employment discrimination in violation of Title VII of the Act. When successful, these lawsuits often resulted in courts imposing affirmative action (AA) plans to increase minority or female representation. By the new millennium, however, the legal environment had become less favorable to AA, and many of the plans had either expired or been successfully challenged as “reverse” discrimination. This paper measures the cumulative causal impact of *temporary* AA on black and female employment at law enforcement agencies. Specifically, we estimate the effects of being sued for discrimination, of operating under an externally-imposed AA plan, and, crucially for long-run outcomes, of emerging from such a plan upon its termination. We study externally-imposed AA plans that address hiring, firing and promotion in law enforcement, exploiting the fact that, although anti-discrimination law applies to all employers, AA was implemented and terminated in a targeted manner in this sector.

We focus on law enforcement for several reasons. First, the variation in timing and outcomes of these cases allows us to determine the long-term effects of temporary AA in isolation from contemporaneous political and social changes in the country. Second, law enforcement was a major locus of Civil Rights litigation, and possibly the sector with the most aggressive externally-imposed AA in US history. For example, in a well-known case, the courts ordered the Alabama State Department of Public Safety to hire or promote one black for every white hired or promoted, until the upper-ranks were at least 25% black.¹ The extensive litigation was due in part to the broad powers given to police, including the right to use force in investigating crimes, apprehending criminals, and maintaining civil order. Potential and actual abuses of these powers by a police force that is not representative of the community it serves can lead to public distrust and violence.² Thus, perhaps more than in other areas, diversity in law enforcement can have social significance and may itself improve performance. These potential quality improvements from diversity provide a third motivation for our focus on police.

The main focus of this study is AA and its termination. This is the first study to investigate the effects of AA termination on employment in general, and police employment in particular. The closest previous paper is McCrary (2007) that shows positive effects of *litigation* on black police employment. We cannot limit our analysis to termination alone and draw directly on the evidence there, as it is limited in several ways crucial to our analysis. First, the data collected for McCrary (2007) does not separate unsuccessful

¹ In 1987, the Supreme Court affirmed the constitutionality of these racial quotas (United States v. Paradis).

² The influential Kerner Commission report argued that police practices contributed to grievances leading to 164 civil disorders and race riots in 1967 and recommended increasing recruitment and promotion of black police officers. In discussing the causes of the riots, the report stated, “to some Negroes, police have come to symbolize white power, white racism and white repression” (National Advisory Commission on Civil Disorders 1968).

litigation from cases leading to externally-imposed AA. If the gains from litigation are independent of the outcome of that litigation, then we should expect no effects from the termination of AA. Second, the employment effects estimated in McCrary (2007) are for all full-time workers, including clerical and janitorial positions. These gains may not reflect increases in black police officers alone. Third, that paper makes no distinction by rank among police officers, although this dimension is of theoretical importance for predicting the effects of termination.

Hence, as a first step toward estimating the cumulative employment effect of AA, we first establish the impact of implementing AA. In this step, we distinguish between litigation and AA and separately investigate employment effects by job type and rank. We otherwise closely follow the empirical specifications of McCrary (2007) to allow for easy comparison of the results. We use employment data from confidential micro-level EEO-4 reports on 479 of the largest US state and local law enforcement agencies, filed with the Equal Employment Opportunity Commission (EEOC) between 1973 and 2005. We searched legal records for each agency in this sample and uncovered 140 cases alleging employment discrimination brought by private plaintiffs or the US Department of Justice (DOJ) between 1969 and 2000 (see Figure 1). These cases comprise our legal database that includes a complete case history of the resulting AA plans formalized in court orders and settlement agreements.³ The bulk of the plans in our sample started during the 1970s, although new litigation and AA continued to be introduced in later decades. About half of the plans had ended by 1993, but some were ongoing in 2005. We use this panel to conduct a dynamic event analysis of the employment effects of key litigation and AA events.

Our first finding is that having an active externally-imposed AA plan results in significant increases in black employment across the ranks of the police hierarchy, over and above any prevailing trends in the country. These increases represent sharp and significant changes in employment trends occurring at the litigation date. Moreover, we uncover important distinctions between being litigated and having an active AA plan. First, departments whose cases do not lead to court-ordered AA still increase their black employment shares, but at a lower rate than departments who are subject to AA. Second, for higher-ranked officers, “litigated only” departments experience a substantially smaller increase in black representation. Employment gains at higher ranks are especially important if access to same-race mentors (Athey, Avery and Zemsky 2000) and role models (Chung 2000) enhances the productivity of new hires.

³ Unfortunately, we have exact information about the use quotas or targets for hiring or promotion in only a few cases, and are not able to use the variation in either the type of plan or whether it applied to hiring or promotion in the estimation.

Changes in public attitudes, state laws, and judicial interpretation have led to a dismantling of AA. Given the current requirement that AA plans be temporary, and not substituted for fair labor practices,⁴ the question of what happens *after* AA termination is of heightened importance. Theoretical predictions are ambiguous. Models are typically characterized by multiple equilibria, some of which predict erosion of gains following the removal of external pressure. Temporary AA can have a lasting impact if, for example, greater exposure to black co-workers eliminates negative stereotypes and reduces taste-based discrimination by employers (Charles and Guryan 2008). Even in models of purely statistical discrimination, permanent gains are possible if AA solves a coordination problem and increases black human capital (Coate and Loury 1993). Temporary AA that increases black representation at higher ranks can have a lasting impact if mentoring both increases productivity and is more effective for racially homogenous pairs (Athey, Avery and Zemsky 2000). Finally, if temporary AA increases the share of blacks involved in hiring or enhances the ability of departments to screen black applicants it can also lead to persistent gains (see Cornell and Welch 1996 and Fryer and Jackson 2008). These models describe potential pathways to persistence, and motivate our empirical investigation. However, as the EEOC data does not include information about the quality of police officers and rejected applicants, we are unable to identify which specific channels lead to persistence in the data.

In our dataset of 140 litigated police departments, 67 experienced AA plans that terminated during the sample period. We use these 67 departments to derive our main results regarding the long-term effects of AA. Compared to departments that were never litigated for employment discrimination, we find no evidence of reduced black employment in the 15 subsequent years. This is especially notable since the plans we study were all externally-imposed as a result of anti-discrimination litigation. Nevertheless, we do find a significant divergence between departments whose plans terminated and those whose plans continued. Prior to termination, the impact of plans that eventually expire is indistinguishable from the employment impact of plans that continue. However, almost immediately after termination, there is a sharp and significant change, and black employment drops significantly relative to departments with ongoing AA.

The estimated 30-year impact of active external AA on black employment is three times the size of the national trend in the period. Since plans typically last fewer than 30 years, we compute the average cumulative effect of actual AA plans on black representation (i.e., the difference between black employment and population shares) during the entire sample period. We find an increase of 4.5

⁴ While the wording of Title VII allows for permanent AA plans, the Supreme Court favored temporary plans as early as 1979 in *United Steelworkers of America v. Weber*. In the 1989 *City of Richmond v. J. A. Croson Co.* decision, the Court expressed this principle explicitly.

percentage points in black representation among lower-ranked workers and of 6.2 percentage points in the higher ranks, over and above the prevailing trends in the country.

The main findings are robust to a variety of alternative approaches to estimating the counter-factual time trends and to controlling for the potential impact of starting and ending court-ordered school desegregation.

We find that female employment shares increase following litigation and AA, consistent with the literature (Sass and Troyer 1999, Lott 2000). However, in contrast to our findings for blacks, the gains for women represent only marginal improvements over national trends. For higher ranks, AA has no residual effect on female employment shares.

This research relates primarily to the literature investigating the effects of Civil Rights legislation and AA on the employment of women and minorities. To the best of our knowledge, this is the first paper to take into account the now temporary nature of AA and to estimate cumulative long-term gains. Previous studies of the 1964 Civil Rights Act and the 1972 Equal Opportunity Act in the private sector have associated voluntary AA plans and federal contractor status with relative gains in minority and female employment (for excellent summaries, see Donohue and Heckman (1991), Holzer and Neumark (2000) and citations therein). While the employment effects of AA have been extensively investigated, the employment effects of AA termination have received little attention.⁵ Fairlie and Marion (2008) investigate the effects of the elimination of voluntary AA in employment in California and Washington. They hypothesize that the opportunity costs of owning a business were lowered for women and minorities as AA elimination worsened their employment prospects. Thus, their rates of business ownership may have increased. In law enforcement, previous research has focused on litigation and AA initiation, but not on termination. As discussed above, McCrary (2007) shows that litigation led to employment gains for full time black workers. While Lott (2000) focuses on crime outcomes, his first stage results suggest positive employment gains for minorities and women from AA plans.

This paper is organized as follows: Section 2 describes the legal and employment database, Section 3 presents results for black employment from a flexible non-parametric event analysis model, Section 4 presents parametric results and robustness analysis for black employment including a discussion of selection into litigation and alternative hypotheses that may explain the findings. Section 5 discusses effects on female employment shares, and Section 6 concludes.

⁵ By contrast, the retreat of AA in higher education has been analyzed in several studies. For example, Long (2004) estimates a significant relative drop in minority applications to top universities in California and Texas, but Card and Krueger (2005) find that highly-qualified minority students still apply. Krueger, Rothstein and Turner (2006) project that, even under optimistic assumptions regarding black test score growth, race-blind admissions 25 years in the future will lead to lower minority enrollment rates at elite colleges. At lower educational levels, Lutz (2005) associates dismissal of court-ordered desegregation plans with increased racial segregation and black drop-out rates. Clotfelter, Vigdor and Ladd (2006) argue federal court decisions hampered continued desegregation in the 1990s.

2. DATA ON AFFIRMATIVE ACTION AND POLICE EMPLOYMENT

2.1 POLICE EMPLOYMENT DATA

We obtained police employment data from the administrative records of the EEOC, collected between 1973 and 2005.⁶ Under Public Law 88-352, Title VII of the Civil Rights Act of 1964, as amended by the Equal Employment Opportunity Act of 1972, all state and local governments with 15 or more employees are required to keep records. Those with more than 100 employees must file EEO-4 reports documenting the number of male and female workers in each racial and ethnic group who fits into each specified cell defined by department function, job function and salary category.⁷ Employers face strong incentives to report accurately: intentional misreporting is a violation of Title 18, Section 1001 of the U.S. Code and punishable by fine or imprisonment. Aggregate data are released to the public. We used confidential individual files, submitted by state and local governments, and identified law enforcement agencies by the *Department Function* for “protective service”.

We grouped law enforcement workers into three categories: full-time (this includes officers, investigators, and support staff), protective (includes patrol officers, deputy sheriffs and detectives) and professional (higher ranking officers such as lieutenants and captains).⁸ Departments are included in the sample if they have at least 200 full-time workers at some point in the sample, have at least 200 protective and professional workers at some point, and appear in the sample for at least 10 years.⁹ Thus, our results apply to larger departments and may not be representative for smaller departments. Unlike McCrary (2007), our sample includes state and local agencies. The results are not sensitive to excluding state departments or smaller local ones.

The EEO-4 reports are only available intermittently before 1985 and for odd-numbered years afterwards. Following McCrary (2007), we used linear interpolation to create a full panel for each department and avoid compositional biases.¹⁰ To prevent this interpolation from artificially increasing the

⁶ We were unable to obtain hand-coded files from 1977, 1975, 1981 or years prior to 1973.

⁷ Information about the EEO surveys is available at <<http://www.eeoc.gov/employers/surveys.html>>. The micro-data are not available to the public. Interested researchers should contact Ronald Edwards at the EEOC.

⁸ Smith and Welch (1984) provide evidence of inconsistency in employment data between the EEO-1 and the CPS and Census data. However, as the sources appear to conflict regarding the distinction between professional and managerial classes, these concerns should not affect our professional category that includes both professional and managerial occupations. Moreover, the authors report that these inconsistencies disappear by 1974 and thus they should be irrelevant for our sample period.

⁹ This rule eliminated about half of the departments in the recent EEOC files and led to an initial database of 446 departments for the legal research. As we encountered legal information on an additional 33 departments that narrowly missed the size criteria (4 of them litigated, 3 with AA), we enlarged the sample to 479.

¹⁰ We also replaced a small set (under 1%) of raw data points that were dramatic outliers in terms of the year-to-year changes in employment shares for a single year only with linearly interpolated values from adjacent years, as we are convinced that they represent transcription or data entry errors.

precision of our estimates, and to allow for arbitrary correlations in errors within departments, we clustered the standard errors at the police department level and adjusted the degrees of freedom accordingly.¹¹

In our analysis of black employment, we followed McCrary (2007) and defined our primary outcome measure as the *Black Representation Gap*: the difference between the percent of police employment that is black and the percent of the local population served that is black. This measure allows us to differentiate between departments with similar black employment shares who serve areas with different black population shares. In addition, the representation gap accounts for racial differences in migration and fertility. In Web Appendix B, we show that black population shares increase more for police departments with AA plans than those without, and that our main results are robust to using black police employment shares as the dependent variable. We obtained the annual population estimates from the Census (CDC Wonder). For municipal and county law enforcement, we used county-year population. For state agencies, we used state-year. *Female Employment Share* is the outcome of interest in Section 5.

2.2 AFFIRMATIVE ACTION CASE HISTORIES

The case history database was constructed by individually querying each of the 479 departments in the police employment sample using both the LexisNexis and Westlaw federal case databases for all documents pertaining to litigation involving sex or race discrimination in employment. We gathered information on the actual AA plan: the litigation, start and (when applicable) end dates, and whenever possible the protected group and the reason for the termination in applicable cases. We contacted individual departments and the DOJ to help complete missing information. The results were then cross-referenced with data from the DOJ on their employment discrimination cases involving police departments, the databases used in McCrary (2007) and Lott (2000), and survey data from the National Center for Women and Policing (2001). Our database expands on these existing sources by compiling a full case history for police employment litigation alleging race or sex discrimination. The legal databases are not complete, and some case details are not available in electronic format. Departments for which we found no information are coded as un-litigated, and litigated departments whose cases were dismissed or for which we found no evidence of AA are coded as litigated only. To the extent that we missed litigated cases or treated some plans as ongoing beyond their termination dates, our estimates will be biased *against* finding significant effects of litigation, AA, and termination. The reason we expect attenuation is not from classical measurement error, but rather the systematic nature of the error introduced by missing

¹¹ We also repeat the regressions in Section 4 using only the limited sample of years for which we observe EEO-4 reports, and the results are unchanged in magnitude or statistical significance. Those years are: 1973, 1974, 1980, 1984, 1985, 1987, 1989, 1991, 1993, 1995, 1997, 1999, 2001, 2003, and 2005.

cases: if present, the true employment gains from AA will contribute to common year effects rather than AA effects, as will the true costs from termination.¹² The estimates will also be attenuated if there are important spillovers in the gains from litigation to nearby un-litigated departments.

Our final dataset includes 479 police departments, of which 117 had court-imposed AA and 23 experienced litigation that did not lead to AA. During the sample period, 67 of the 117 AA plans were terminated. Six additional plans ended by 2008.¹³ The histogram in Figure 1 illustrates the time pattern of litigation and AA. Half of the plans resulted from litigation brought before 1980, and half ended by 1993.¹⁴ Among plans with known end dates, the mean duration was 16 years. Among plans in which the protected category can be determined, 96% involved blacks and 68% involved women. The results below are estimated on the sample of all plans. If we only examine cases involving blacks in Sections 3 and 4 or women in Section 5, the results are unchanged.

Table 1 reports mean values of key variables, separately for each of the four types of departments: never litigated, those whose litigation does not lead to AA, those with externally-imposed AA without a set end date, and those with temporary externally-imposed AA. Most departments in the sample were not litigated, and most of the observed litigation ended in AA. The South and Northeast are over-represented among litigated departments. Litigated departments have more full-time employees. Those with AA are located in areas with higher black population shares and lower schooling. In terms of the main employment outcomes, “litigated only” departments had higher black representation in 1973 than un-litigated ones, while those whose litigation ended in AA had substantially worse representation gaps in all three job types. By 2005, however, litigated-with-AA departments had similar, and generally higher, representation gaps than un-litigated ones. This differential trend suggests that AA played a role in

¹² Our legal research is more likely to uncover the end date for court-ordered plans of limited terms or plans that were challenged in “reverse discrimination” suits. Thus there is a potential bias in terminated cases. However, we find no significant differences in either the effects of having an AA plan or of terminating such a plan between plans that were ended (about 33%) by the courts and those that were allowed to expire.

¹³ Many plans include expiration dates in the original court order or settlement agreement. We assume that a plan ends when it expires. Some plans were modified and continued beyond their original expiration dates; in those cases, we use the ultimate end date. Other plans were terminated early as a result of “reverse discrimination” suits, brought usually by white males. In those 25 cases (all but one ending after 1988), the actual end date is used. Finally, there are cases involving the DOJ for which we found no court record of an end date. When applicable (21 cases), we use the DOJ’s internal end date, when the active file in their records was closed. Conversations with the Civil Rights Division confirmed that cases with active AA plans remain open until the plan ends. However, there may be a lag between the end date of the plan and the internal close date at Justice, which may influence our estimates. We confirm that our main results are unchanged, by repeating the regressions in Section 4.1 allowing for different trends after termination for departments with publicly observed end dates and departments with DOJ end dates.

¹⁴ In unreported regressions, we find evidence that earlier cases have significantly larger effects on black employment, but we find no differential effects on black employment by AA termination year. Though it is impossible to determine the precise cause of the former, we find several observable characteristics of the earlier plans (longer duration, higher black population share and worse initial representation gap) contribute to the effect.

increasing black police employment. The rest of the paper will exploit variation in the exact timing of litigation and AA termination to determine how much of this relative increase is attributable to court-ordered AA, and to assess the durability of any gains beyond the period of active external monitoring. In contrast, foreshadowing the results in Section 5, female employment shares were quite similar across the different department types both at the start and end of the sample period.

3. EFFECTS OF LITIGATION AND AFFIRMATIVE ACTION ON BLACK POLICE EMPLOYMENT

3.1 NON-PARAMETRIC ESTIMATION MODEL

We first estimated a flexible non-parametric model of the dynamic effects of litigation and AA, including leading and lagging effects around three key events: 1) litigation not leading to AA, 2) litigation leading to AA and 3) the termination of AA. Several sources of variation provide identification. First, there are 4 types of departments in our sample: never litigated; litigated but without externally-imposed AA; litigated with an AA plan that expired by 2008;¹⁵ and litigated with an AA plan with no known end date. Next, litigated departments varied in their timing of litigation. Finally, those with AA end dates experienced varying timing and duration of AA.

The unit of observation in our panel dataset is a police department i in a year t . We constructed 3 variables to measure time before and after each of the key events: $YearsAfterLit(NoAA)_{it}$, $YearsAfterLit(AA)_{it}$ and $YearsAfterEnd_{it}$. The range of values for each of these variables depends on the department in question. For an un-litigated department such as Tucson, Arizona, these variables are set to zero in all years. For a litigated department without AA, such as Fort Wayne, Indiana (litigated in 1980), only $YearsAfterLit(NoAA)_{it}$ varies (it ranges from -7 in 1973 to 25 in 2005). Departments with AA of an indefinite duration, such as White Plains (litigated in 1980), only $YearsAfterLit(AA)_{it}$ varying (it ranges from -7 in to 25). Finally, for departments with observable AA end dates, such as the Ohio State Highway Patrol (litigated in 1980, AA ended in 1988), $YearsAfterLit(AA)_{it}$ and $YearsAfterEnd_{it}$ vary (ranging from -7 to 25 and -15 to 17, respectively). Before the termination date, the $YearsAfterEnd$ variable is divided into two distinct time periods: litigation date and afterwards (ranging, for Ohio State Highway Patrol, between -8 to -1) and before litigation date (ranging from -15 to -9). Under this definition, $YearsAfterLit(AA)_{it}$ continues to increase in the years after AA ends. We use the litigation filing date for cases that end in AA, rather than the date of the consent decree or final judgment. This enables a comparison between the effects of litigation alone and of litigation leading to AA, and is consistent with the non-parametric estimates for full-time and protective workers, presented below, indicating employment practices started changing immediately after litigation.

¹⁵ We group together departments with plans that end during and after the sample period in order to capture possible differences in the effects of active AA between plans that are known to end and those without known end dates.

Our non-parametric model includes indicator variables for each of the 10 years preceding the litigation events, and each of the 30 years following them. For termination, the variables range from 20 years before to 15 years after. We grouped the years outside these ranges with their closest endpoints to avoid estimating separate coefficients for rare events. We estimated the model below using OLS separately for each employment categories: full-time, protective, and professional.

$$\begin{aligned}
BlackRepGap_{it} = & \sum_{j=-10}^{-1} \beta_{NoAA,j} 1(YrsAfterLit(NoAA)_{it}, j) + \sum_{j=1}^{30} \beta_{NoAA,j} 1(YrsAfterLit(NoAA)_{it}, j) + \\
& \sum_{j=-10}^{-1} \beta_{AA,j} 1(YrsAfterLit(AA)_{it}, j) + \sum_{j=1}^{30} \beta_{AA,j} 1(YrsAfterLit(AA)_{it}, j) + \\
& \sum_{j=-20}^{-1} \beta_{End_BeforeLit,j} 1(YrsAfterEnd)_{it}, j | t < LitYr) + \sum_{j=-20}^{-1} \beta_{End_AfterLit,j} 1(YrsAfterEnd)_{it}, j | t > LitYr) + \\
& \sum_{j=1}^{15} \beta_{End,j} 1(YrsAfterEnd)_{it}, j) + \alpha_i + \tau_t + \varepsilon_{it}
\end{aligned}$$

The main effects of interest are captured in the vectors of β coefficients on the indicator variables for the number of years before and after each of the key events. The indicator variable function $1(.,.)$ takes a values of 1 when its two arguments are equal and zero otherwise.¹⁶

In order to separate the policy effects from permanent department-specific factors that affect representation gaps, such as location or preferences, we included a full set of α_i variables for department fixed effects. We also controlled for arbitrary non-linear national trends in representation gaps using the τ_t calendar year fixed effects. These controls make the model analogous to a difference-in-differences, and each of the parameters of interest can be interpreted as a cumulative change in the representation gap, for a department exposed to a particular policy, relative to a base year and a comparison group.

Since different departments are sued in different years, both litigated and un-litigated departments contribute to the estimates of year fixed effects. Litigated departments without AA experience these common time trends, and also deviations from these trends due to litigation, expressed in the $\beta_{NoAA,j}$ terms. Differential trends leading up to litigation are measured by $\beta_{NoAA,j}$ for negative values of j ; a positive pre-trend would appear as negative point estimates that increase in magnitude as j approaches zero. Differential trends following litigation are measured by $\beta_{NoAA,j}$ for positive values of j . For departments with AA that does not end, the base year is the year of litigation and the comparison for time trends is changes in the rest of the country. Since the model includes a set of $\beta_{End_AfterLit,j}$ estimates for years before AA termination and after litigation, the $\beta_{AA,j}$ parameters should be interpreted as the trend for departments with AA and no end date. The trend during active AA plans for those that end can be

¹⁶ For example, the variable $1(YrsAfterLit(NoAA)_{it}, 1)$ is set to 1 in the year after litigation for departments whose litigation does not lead to AA. In other years, and for other departments, the variable is zero.

computed for each department by summing the relevant $\beta_{AA,j}$ and $\beta_{End_AfterLit,j}$ estimates. This setup incorporates the possibility that AA has different effects for plans that ended and those that continued. By estimating $\beta_{NoAA,j}$, $\beta_{AA,j}$, and $\beta_{End_AfterLit,j}$ coefficients, we are also able to measure how the effects of litigation vary depending on the outcome of the case.

When evaluating the changes in representation gaps following AA termination, the $\beta_{End,j}$ values should be interpreted as relative to the end year and relative to changes in departments in which AA continues, for which the same number of years has passed since litigation. This first comparison allows us to determine if the gains from AA continue at the same rate after the plan is removed. Another important counter-factual is how the post-termination changes compare to changes in un-litigated departments. In Section 3.3 below, we accomplish this by estimating the non-parametric model with a second definition of $YearsAfterLit(AA)_i$: instead of increasing in the years after termination, the variable retains its value in the end year. The $\beta_{End,j}$ values can then be interpreted as the changes in representation gaps following AA termination, relative to the end year, and relative to trends in the rest of the country. This second comparison is crucial for determining if departments revert to previous practices once the external pressure is removed.

3.2 NON-PARAMETRIC RESULTS: EFFECTS OF LITIGATION AND ACTIVE AFFIRMATIVE ACTION

The estimation results for the effects of litigation and active AA (relative to no litigation) are presented in Figure 2, separately for each of the job categories. Panel A presents the $\beta_{AA,j}$ coefficients on each of the years before and after litigation for departments that underwent AA, while Panel B shows the related $\beta_{NoAA,j}$ coefficients for litigation that did not lead to AA. The 3 rows correspond to the job categories full-time, protective, and professional. The point estimates are depicted with diamonds, surrounded by bars marking the 90 percent confidence intervals around each estimate. Externally-imposed AA plans started in the early 1970s, and our sample includes over 40 departments with 30 or more years since litigation by 2005.

Several key patterns emerge from these figures. First, the positive and growing post-litigation estimates indicate that blacks experience large employment gains in the years following litigation.¹⁷ Since the model includes calendar year fixed effects, the gains depicted in the figures are net of any national trends towards increasing black representation in policing. Each of the point estimates for different years

¹⁷ Since the $\beta_{End_AfterLit,j}$ coefficients (presented below) are not significantly different from zero, the estimates in the figure apply equally to departments with AA that does and does not end.

after litigation represents a difference-in-differences that is averaged over the set of departments whose plans last at least that long.¹⁸

Second, although litigation alone *does* affect full-time and protective employment, it has a smaller estimated effect than litigation leading to AA. This is apparent in the comparison between Panels A and B of the figure. In the 30 years following litigation, departments with AA plans increased their black representation among full-time and protective workers by about 10 percentage points and among professional workers by about 15 percentage points. By contrast, departments who were litigated, but did not have court-imposed AA plans increased their black full-time and protective representation by 5 and 7 percentage points, respectively, but failed to increase their black professional representation by a statistically significant amount (the point estimate at 30 years is less than 2 percentage points). This suggests that formal plans had a greater impact than litigation alone, and that formal plans were vital in order to enable blacks to penetrate the higher levels of command in departments accused of employment discrimination.

In addition to these larger effects from AA, the professional group also differed from the protective group in the timing of the gains. While for protective workers, the point estimates are positive for all years after litigation, the gains for professionals appear 5 years after litigation. This pattern may suggest that the increased hiring of blacks at the lower ranks contributed to enlarging the pool of black officers who were candidates for internal promotion to higher ranks. Although the number of years between initial hiring and promotion to a professional rank such as lieutenant varies across departments, a period of more than 5 years is reasonable. While the point estimates for professionals are 1.5 times those for protective workers, the professional gains can still be explained by the blacks hired at lower ranks as a result of AA being promoted to higher ranks. This is because the average number of employees in the protective category is more than 3 times the number in the professional category. Among AA departments in our sample, the average numbers of protective and professional workers are 1240 and 382, respectively.

The estimated effects of externally-imposed AA are substantial. The 30-year gains of 10 to 13 percentage points are larger than the overall trends during the period. The year fixed effects indicate that

¹⁸ As the number of years following litigation increases, the set of departments is limited to those with longer durations. Hence, even if all the gains were immediate, the figure would have a positive slope if departments with longer AA plans experienced greater gains from AA. To rule out the possibility that the apparent increases in the figures are driven solely by compositional shifts, we re-estimate the model on a balanced set of departments. To do so, we omit departments whose plans lasted fewer than 15 years by 2005 (excludes 42 departments) and those who were litigated prior to their entry into the sample (eliminates another 18 departments). The estimates up to 15 years after litigation are now derived from a balanced set of departments, shown in Web Appendix A Figure WA1. For each employment category, we find a pattern of increasing gains, suggesting that the increasing patterns in Figure 2 are not driven by compositional shifts, but rather form a continuous increase in black employment. Interestingly, Figure WA1 estimates are somewhat higher than that those in Figure 2, suggesting that longer plans are associated with larger gains. However, these differences are not statistically significant.

the black full-time representation gap improved by 2.6 percentage points in the 30 years between 1973 and 2003. For protective and professionals, the changes were 1.7 and 2.9 percentage points, respectively. The national trends, after removing the effects of litigation and AA, were towards increasing black representation in full-time employment until the early 1990s, in protective employment until the mid-1990s, and in professional employment throughout the period. Average black population shares in AA departments went from 17 percent in 1973 to 23 percent in 2005, so police employment shares increased even more.

The third important feature of Figure 2 is its depiction of differential trends in black representation gaps in the years prior to litigation. For departments whose litigation ended in AA, there is no evidence that the apparent gains following litigation were merely the result of pre-existing department-specific trends towards increasing black employment. If anything, these departments exhibit a relative deterioration in black representation gaps (mainly for protective) in the years leading up to litigation.¹⁹ This distinguishes them from departments that were sued unsuccessfully, who exhibit a weak pattern of improvement before litigation. For those departments in Panel B, the changes in trend around the time of litigation are far less positive than for AA departments. The sharp break from trend among AA departments, centered on the litigation year, provides strong support for a causal role for the legal intervention.

3.3 NON-PARAMETRIC RESULTS: EFFECTS OF ENDING AFFIRMATIVE ACTION

We now consider two important counter-factual comparisons to assess the effects of AA termination. The first comparison is between what happened around the end date and what would have happened if AA had continued. This is estimated by comparing actual trends around the end year to trends experienced in departments with the same time elapsed since litigation whose AA plans remained in place. The second comparison is with un-litigated departments, and involves comparing trends around the end year with the calendar year trends exhibited by all departments in the country. These estimates are in Panels A and B of Figure 3, respectively.

Panel A plots the $\beta_{\text{End_AfterLit},j}$ estimates for values of j ranging from -20 to -1 and the $\beta_{\text{End},j}$ estimates for values of j ranging from 1 to 15. The rows correspond to the job categories full-time, protective, and professional. The point estimates are depicted with diamonds, surrounded by bars marking the 90 percent

¹⁹ This pattern brings to mind the “Ashenfelter dip” (Ashenfelter 1978) in earnings in the period prior to entry in training programs. A potential concern arises if departments are litigated following a temporary period of declining black representation (absolute or relative to the country), from which they would have recovered even without litigation or AA. The “dip” observed in our data it is not a single period shock, but a trend that becomes significant over a period lasting more than five years. Thus, there is little reason to expect the recovery to coincide sharply with start of external pressure, as shown in the figures. Indeed, when we add controls for mean-reversion and lag-dependence more generally using the parametric model in Section 4.1 the main coefficients are unchanged.

confidence intervals. Because the $YearsAfterLit(AA)_{it}$ variable continues to increase after termination, the comparison is between departments that ended and continued AA after the same number of years since litigation. Since the models include department and year fixed effects, as well as controls for years before and after litigation, the estimates should be interpreted as changes in representation gaps, relative to the base year in which AA ended, and relative to departments in which AA continued. The estimates for years preceding the termination date are generally small and never statistically significant. This implies that, during the period of active AA, the estimated gains from AA were the same for departments with AA that ended as for departments whose AA continued. Although we estimated the full vector of $\beta_{End,j}$ coefficients, the pre-litigation effects are not important. While there is some suggestion of relative increases in the representation gaps for full-time and protective workers in the second decade before termination, the trend differences in years closer to termination are negligible.

In contrast to the period before AA termination, we observe a significant divergence in outcomes afterwards. For all three categories of workers, the negative and declining estimates show that gains were smaller after AA termination than they were when AA continued. The relative declines are large, over 4 percentage points in the 15 years after AA, and the point estimates are statistically significant (almost immediately for full-time and professional, and after about 7 years for protective). Consistent with the finding that AA was most important for professional gains (relative to no litigation and to litigation only), we find that the end of AA is associated with the largest relative losses for professionals. The decline over 15 years is over 7 percentage points and within only a few years it is statistically different than zero. These relative declines show that one-shot exposure to externally-imposed AA was not sufficient to accrue maximal gains. When the external pressure was removed, the gains did not continue at the same pace as when it was applied.

For the years following AA termination, Panel B of Figure 3 plots the $\beta_{End,j}$ estimates for positive values of j from a modified version of the non-parametric model. Instead of allowing the $YearsAfterLit(AA)_{it}$ variable to increase after AA termination, it is capped at its value in the end year. This value corresponds to the time elapsed between the litigation year and the termination date of the plan, and is roughly equal to the duration of external scrutiny and AA for that department. The post-termination coefficients can thus be interpreted as the average difference between the changes in the representation gap between the end year and the current year, less any changes during that calendar year period in unlitigated departments. The pre-termination coefficients and confidence intervals are computed from linear combinations of $\beta_{End_AfterLit,j}$ and $\beta_{AA,j}$ estimates to create differences-in-differences between the current

year and the end year, again relative to un-litigated departments.²⁰ The significant and increasing trend observed before AA termination corresponds to the estimated gains following litigation for departments ordered to implement AA. The new information in this panel is the lack of a significant trend following AA termination.²¹ The previously observed gains are halted at the end date, but there is no suggestion of any erosion or reversal following termination. These new findings imply that temporary externally-imposed AA plans increased black representation gaps, and that the gains lasted at least a decade and a half beyond the end date of the plan.

3.4 NON-PARAMETRIC RESULTS: CUMULATIVE EFFECTS OF TEMPORARY AFFIRMATIVE ACTION

In Section 3.2, we presented the average changes in black representation gaps between the year of litigation leading to AA and each of the next 30 years after litigation. In assessing the historical impact of AA in US law enforcement, however, it is essential to combine these estimates with information about the distribution of durations of actual plans. Within our sample of departments with externally imposed AA plans, we observe 67 plans that end prior to 2005. These departments experienced AA lasting between 3 and 30 years, with an average duration of 14 years, resulting from litigation that occurred between 1970 and 1994.

In the previous section, we showed that the gains following litigation are indistinguishable between plans that ended and those that did not: $\beta_{\text{End_AfterLit},j}$ is never significant. Hence, we estimated a simplified version of the non-parametric model with $\beta_{\text{End_AfterLit},j} = 0$ and $\beta_{\text{End_BeforeLit},j} = 0$ and used that model to calculate the average cumulative effects of the AA plans in the sample at the time of their termination. We computed a linear combination of the $\beta_{\text{AA},j}$ parameter estimates for positive values of j representing the total duration of each terminated plan. The estimates of cumulative gains are: 2.3 percentage points (s. e. 1.1) for full-time workers, 2.4 percentage points (s. e. 1.1) for protective and 3.2 percentage points (s. e. 1.3) for professional. Since we are also interested in the cumulative effects of AA in all affected departments during the sample period, we estimated another version of the average cumulative effect. This second average includes cumulative effects up to the termination date for plans that ended, as well as the cumulative gains from litigation until 2005 for the 50 plans that are still active at that time (average

²⁰ The formula for pre-termination coefficients is: $\gamma_{-j} = \beta_{\text{End_AfterLit},-j} + \frac{1}{N_{-j}} \sum_{i=1}^{N_{-j}} \beta_{\text{AA}, \text{Duration}_i - j}$, where N_{-j} is the number of departments observed j years before termination, Duration_i is the total number of years between litigation and termination for department i . Standard errors are calculated using the delta method.

²¹ This absence of a trend in the years after termination makes the unstable composition of the sample less of a concern. Regardless, we re-estimated the model on the balanced sample of departments whose plans ended at least ten years before the end of the sample. This excludes 20 departments. The resulting figures (Web Appendix A Figure WA1, Panel B) also show no significant post-termination trends relative to un-litigated departments.

duration of 19 years). The estimates of cumulative gains are larger: 4.5 percentage points (s. e. 1.4) for full-time, 4.5 percentage points (s. e. 1.4) for protective and 6.2 percentage points (s. e. 1.7) for professional.

4. PARAMETRIC ESTIMATES AND ROBUSTNESS ANALYSIS

The non-parametric estimates in Section 3 established the main results for black representation gaps. In this section, we confirm the break from trend using a linear model for years before and after key litigation and AA events. We then test the robustness of the relationships to alternative specifications and evaluate the role of court-ordered school integration on black employment.

4.1 PARAMETRIC MODEL AND BASELINE ESTIMATES

The parametric model is the linear analogue of the non-parametric one presented in Section 3.1:

$$\begin{aligned} \text{BlackRepGap}_{it} = & \beta_{YBL(AA)} \text{YearsBeforeLit}(AA)_{it} + \beta_{YAL(AA)} \text{YearsAfterLit}(AA)_{it} + \beta_{YAE} \text{YearsAfterEnd}_{it} \\ & + \beta_{YBL(NoAA)} \text{YearsBeforeLit}(NoAA)_{it} + \beta_{YAL(NoAA)} \text{YearsAfterLit}(NoAA)_{it} + \alpha_i + \tau_t + \varepsilon_{it} \end{aligned}$$

where the unit of observation is a police department i in a year t . Trends beyond national trends (the τ_t vector) in the black representation gap before and after litigation that does not lead to AA are measured with $\beta_{YBL(NoAA)}$ and $\beta_{YAL(NoAA)}$. Differential trends for litigation leading to AA are measured with $\beta_{YBL(AA)}$ and $\beta_{YAL(AA)}$. The variables tracking years before litigation are assigned negative values that increase towards zero for the litigation year and remain at zero beyond. Years after litigation are zero until litigation, then increasing. As in the original non-parametric model, the variable $\text{YearsAfterLit}(AA)_{it}$ continues to increment in the years following AA. Since the non-parametric results showed no evidence of a differential trend prior to AA termination, we estimate a differential trend following termination ($\text{YearsAfterEnd}_{it}$) but not preceding it.²²

Results from this model are presented in Table 2, separately for each of the job category, and along with estimates from a model without the year fixed effects. The negative point estimates for linear trends before litigation indicate that departments experienced relative declines in their representation gaps in the years between 1973 and the litigation date. The positive estimates for years after each type of litigation echo the non-parametric results in Figure 2. The estimates with year fixed effects (Columns 2, 4, and 6) are smaller than those without (Columns 1, 3, and 5), which is consistent with the generally increasing representation gaps in the country for much of the sample period. For full-time, protective and professional workers, we estimated an average increase in black representation of about 0.4 percentage

²² We also estimate an extension of the model above with additional controls for $\text{YearsBeforeEnd}_{it}$, separating the pre-trend into years before and after litigation. In no case are these coefficients themselves statistically significant, and they do not affect the main results. Interested readers can find those results in the Web Appendix A Table WA1.

points per year after litigation leading to AA (shown by the *YearsAfterLitigation* (AA) coefficients). These gains are significantly different from zero (trends are significantly different from un-litigated departments) and from the pre-existing trends for those same departments before litigation (shown by the results of F-tests on $YearsBeforeLit$ (AA) = $YearsAfterLit$ (AA)). Litigation not leading to AA increased black representation for full-time and protective, but not for professional worker (shown by the *YearsAfter Litigation* (No AA) coefficients), and the gains are significantly smaller than the gains associated with AA (shown by the results of F-tests on $YearsAfterLit$ (AA) = $YearsAfterLit$ (No AA)). The negative and significant β_{YAE} coefficients imply that the increases in black representation gaps are 0.3 to 0.5 percentage points lower after AA termination than they would have been if AA had continued. In the linear framework, we can compare trends following termination to those in un-litigated departments by simply summing the $\beta_{YAL(AA)}$ and β_{YAE} coefficients. These values are close to zero and statistically insignificant (shown by the results of F-tests on $YearsAfterEnd + YearsAfterLitigation = 0$). Hence, the parametric estimates following AA termination date capture the essential features of the non-parametric estimates presented in Figure 3.

The main results are thus established in the parametric model. Externally imposed AA plans are strongly associated with an increase in black employment in all police job categories. The termination of such plans resulted in lower gains relative to departments in which AA plans did not end. Nevertheless, relative to national trends, the termination of AA plans did not result in decreased black employment. Moreover, there are strong indications that these associations represent causal relationships. In particular, the significant changes in trends occurring at the litigation and termination dates suggest that it is the initiation of a plan and its termination that cause the changes in black employment. In addition, the gains in black employment during active AA are significantly larger than the gains occurring after litigation alone, especially for higher ranking police officers in the professional category. This suggests that it is the plan, and not the particular environment in the police department, that is responsible for the observed gains in black employment. In the next section, we provide the results of several robustness checks that reaffirm these estimated effects of externally-imposed AA and its termination.

4.2 ROBUSTNESS: ALTERNATIVE COUNTERFACTUAL TIME TRENDS

This section presents various robustness checks for the estimated effects of litigation and AA on employment. The model in Section 4.1 uses year fixed effects to capture the non-linear trends in representation gaps that are common to all departments in the sample. However, it is important to note that litigation was not random across departments. Table 1 shows that litigated and un-litigated departments differed in location, size, and the proportion of the local population that is black. If these

characteristics are associated with differences in underlying time trends, the regressions assuming a common trend may produce biased estimates of the impact of AA.

We used two empirical approaches to address concerns regarding the appropriate counter-factual time-trend for what would have happened to litigated departments in the absence of legal intervention.²³ First, we estimated a model that replaces the year fixed effects with year-Census division fixed effects. The results are reported in columns 1, 3 and 5 of Table 3; the main estimates are essentially unchanged. Columns 2, 4 and 6 of the table show the results are also robust to including a full set of state-year interactions, although the post-litigation trends for AA and non-AA departments are not statistically distinguishable for full-time or protective. Since the within-region state-year interactions are not generally important (with the exceptions of trends in West North Central, South Atlantic and Pacific regions) and their inclusion relies on some small comparison groups that may be unreliable for estimating the non-linear time trends, our preferred specification uses region-year interactions. The non-parametric results with the preferred region-year fixed effect are virtually identical to those with year fixed effects alone. Figures WA2 and WA3 in the Web Appendix A provide these estimates. We use that specification in what follows.

Our second approach was to limit the sample to litigated departments and to estimate the counter-factual time trend using only litigated ones, thus eliminating any bias from unobservable differences between litigated and un-litigated departments not controlled for in other robustness exercises. Even with the variation in start and end dates, we cannot estimate the full model with flexible time trends, department fixed effects and separate trends before and after AA events. To avoid altering the interpretation of the main coefficients, we chose to retain the full set of year-region indicators, and to continue to estimate separate trends for litigation leading to AA and not leading to AA. We avoided colinearity by redefining the litigation year to include the years immediately before and after it. This grouping preserves the interpretation of the slope change as centered around the litigation year, and is consistent with the non-parametric estimates that showed no discrete jumps immediately before or after litigation. Using this definition on the full sample has no effect on the main estimates (see Table WA2). The results for the litigated sample are in Table 4 and again confirm the empirical findings of the previous methods. These estimates are somewhat larger than those from the full sample, which implies that the

²³ In Web Appendix B we report two more approaches that allows for other observable characteristics of departments (besides geographical differences in time trend) to be associated with both the probability of being litigated and with the underlying time trend. The results of these models confirm our main findings. We also show that our main results are robust to using black police employment shares as the dependent variable.

main results are not driven by departments under AA hiring blacks away from neighboring un-litigated ones.²⁴

4.3 ROBUSTNESS: ALTERNATIVE HYPOTHESES

This section considers potential explanations for the main findings of the paper that involve factors other than litigation and AA termination directly causing these changes.

First, we consider the alternative theory that the gains during AA are in fact causal, but that the slow-down following AA termination relative to departments in which AA continues is only coincidentally related to the timing of termination. This is superficially plausible if the costs of increasing black police employment also increase with the representation gap, and the costs of additional increases become prohibitive above some natural representation gap level. This mechanism can generate the apparent decline in gains at the time of AA termination if departments had high representation gaps in their termination years. First, it is important to note that the average representation gaps in AA end years were well below equal representation, an obvious candidate for the natural level above which increases are unlikely. The average end year gaps were -3.5 for full-time, -4.1 for protective and -6.7 for professionals.

In order to assess the importance of this mechanism, we categorized each of the 67 departments with end years during the sample period according to their black representation gaps in their AA end year. Departments with representation gaps above the median for departments with ongoing AA in that calendar year were classified as *HighRepGap*.²⁵ We re-estimated the parametric model and allowed for heterogeneous effects by interacting the *HighRepGap* indicator with *YearsAfterEnd*. Results with region-by-year fixed effects are in the first three columns of Table 5. Under the mechanism in question, departments with representation gaps below the median should be more likely to experience continued gains following termination. Instead, these departments exhibit post-termination trends that are statistically indistinguishable (for full-time and professional) from those in departments with above median gaps. For protective workers, the post-termination gains are significantly larger in *HighRepGap* departments. Furthermore, the below-median departments show statistically significant declines in representation gaps for all worker types following AA termination, relative to departments with continuing AA.

Finally, we consider an alternative hypothesis that could explain the apparent persistence of the employment gains from temporary AA, even if hiring practices following termination in fact reverted to

²⁴ Further evidence that AA did not merely shift the work location of a fixed pool of black officers is the positive time trend in the un-litigated departments throughout most of the period. This is consistent with the negligible spillover effects on un-litigated departments in McCrary (2007).

²⁵ This applies to 43 percent of departments for full-time, 48 percent for protective and 49 percent for professional.

pre-AA patterns. Under this hypothesis, temporary plans produced only temporary gains, but the time scale for erosion is longer than that for gains. One reason to expect that black employment gains during AA would occur at a faster rate than the reversal of those gains following AA is that more senior employees were more likely to be white throughout the period. Thus, we expect whites to retire at higher rates than blacks. If a department reverts to a hiring pattern that is substantially less representative than its hiring before termination, but that still resembles the racial composition of the exiting population, the effect in the short run will be a leveling of gains. An absolute decline in black employment shares will only occur in the long run. This story does not apply to growing departments, where black hiring shares will immediately shift black employment shares, as long as they differ from current black employment shares. In growing departments, there is no time delay between reductions in black hiring and reductions in overall black employment, even if exit rates do differ by race.

In fact, average department size in our dataset increased during the period following AA termination and over 60% of those with end dates were expanding. Nevertheless, we re-estimated the post-AA trends separately for expanding departments and found no relative decline after termination relative to unlitigated departments (see the last three columns of Table 5). There is evidence that departments whose size remained level or decreased experienced smaller drops in black representation immediately after AA termination for protective workers. While the slower decline is consistent with a role for differential retirement rates, this factor alone cannot explain the finding that the gains from AA persisted well beyond its termination year.

4.4 COURT-ORDERED SCHOOL DESEGREGATION

In this section, we explore the relationship between court-ordered public school desegregation and the police employment outcomes of interest. School desegregation was another large scale Civil Rights policy intervention that may have potentially influenced police employment during the sample period. Previous studies have linked externally-imposed desegregation plans to improved educational outcomes for black students (see Reber 2005 and Guryan 2004). These improvements in human capital could in turn lead to increased employment shares in local police departments. Like court-ordered AA for police hiring, court-ordered school desegregation was implemented at different times in different places, resulting from individual court cases against specific localities. Court-ordered desegregation plans were introduced during the same period as court-ordered AA plans, though they started somewhat earlier (nearly 20% started prior to 1970, and almost 90% started before 1980). As with AA, shifts in the legal environment led to the termination of many school desegregation plans, primarily during the 1990s. Many of the same cities that experienced external police AA also underwent external school desegregation. In the estimation sample, the South Atlantic Census division has the most plans of either type.

The previous results and robustness analysis controlled for time trends that were national, regional, or common to departments with similar observable characteristics. These approaches will not remove the effect of school desegregation if common unmeasured features (for example, black political power) made certain cities more likely to be litigated for both police and school diversity at around the same time. Additionally, school desegregation itself may be the source of our previous findings. In order to further isolate the effects of police AA on black representation gaps, we estimated an augmented version of the parametric model with 4 additional controls for school desegregation: *Years Before Desegregation Start*, *Years After Desegregation Start*, *Years Before Desegregation End* and *Years After Desegregation End*.²⁶

The first result to emerge from this analysis is the estimated effects of starting and ending police AA are unchanged. Table 6 reports estimates for models with year fixed effects and with year-region interacted fixed effects. The dependent variables are the three measures of the police representation gap for full-time, protective and professional workers. Table 6 shows that including the variables that account for desegregation and its end leave our main results unchanged. Thus, we can rule out the possibility that either school desegregation itself or unobserved characteristics alone are responsible for our main results.

The second finding in Table 6 is a positive association between the presence of court-ordered school desegregation plans and black representation in police employment. The *Years After Desegregation Start* estimates are positive in all models and significant in models with year fixed effects for protective and professional and year or region-year effects for full-time. This association may suggest that the improved human capital of blacks educated in cities following desegregation led to improved labor market outcomes. Alternatively, racial integration in schools may have changed negative perceptions regarding blacks or court-ordered desegregation may have occurred at the same time as changes in the political environment that also affected police employment. The negative relationship of police representation gaps with the termination of school desegregation plans fits the same pattern. The large and significant effects on the professional category of workers especially may indicate that the court-ordered end of desegregation was associated with changes in the political environment that also affected police.²⁷ The impact of ending desegregation may be attributable to these unmeasured factors.

²⁶ School desegregation dates are from: Guryan (2004), Reber (2005), Lutz (2005), and Weiner et al. (2006).

²⁷ To explore these issues further, we re-estimate the Section 3.1 non-parametric model adding dummies to account for years before and after desegregation start and end. The results suggest desegregation has a significant positive impact on police employment only after 25-30 years. Thus, it seems unlikely that these relationships result from the contemporaneous political environment. However, they may be related to either human capital accumulation or to improved public perception of the labor market skills of blacks and elimination of negative stereotypes. The non-parametric estimates suggest police employment is reduced about 5 years after re-segregation, consistent with the hypothesis that the relationship relates to concurrent political changes.

5. EFFECTS OF AFFIRMATIVE ACTION ON FEMALE POLICE EMPLOYMENT

The previous sections of this paper demonstrated a causal relationship between the imposition of AA plans on police departments and subsequent changes in black representation gaps for each of the job types. The measured gains following the installation of AA plans represent significant improvements over national and regional trends for un-litigated departments. This section describes the non-parametric estimates for the changes in female employment shares in the years before and after police employment litigation leading to AA.

We used the same flexible non-parametric model presented in Section 3.1 to estimate the dynamic effects of litigation and AA, changing the dependent variable to female employment share. Figure 4 plots the series of $\beta_{AA,j}$ terms from the non-parametric model with the full set of leading and lagging trends. Point estimates from a model with year fixed effects are shown with diamonds (surrounded by 90% confidence intervals) and those without fixed effects are shown with crosses. The dependent variables are black representation gaps for the three job types in Panel A, and female employment shares in Panel B. Focusing on outcomes for women, it is evident that litigation leading to AA was followed by dramatic increases in female employment in each of the job categories, especially high-ranking professional jobs. However, the bulk of these gains were also experienced in un-litigated departments: inclusion of simple year fixed effects eliminates the apparent gains in professional jobs and dramatically reduces the estimated gains for full-time and protective. By contrast, the inclusion of year fixed effects has only small effects on point estimates for black representation gaps, and the confidence intervals are largely overlapping in Panel A. As the effects of starting AA on female employment appear unimportant, we do not report the estimated effects of terminating AA programs. These are, not surprisingly, negligible. We find similar results when we restrict the litigated sample to cases that involve women as the protected group. Leonard (1984) reports a similar pattern in the federal contractor program: large gains for black men and women, but small gains for white women.

Thus, we find that AA in law enforcement had differential effects on the two protected classes of workers. AA had sizable positive effects on black employment, over and above the national trends captured by year and year-region fixed effects. For women, the effects of AA were small and swamped by the national trends. This difference may be driven by the dramatic increases in female, but not black, labor supply during the period, or it may be that specific aspects of AA and anti-discrimination law affected sex and race differently.²⁸ The frailty of the results for sex serves to highlight the robustness of the results for race.

²⁸ An example is the 1977 Supreme Court ruling in *Dothard v. Rawlinson* (433 U.S. 321) that rendered height and weight standards illegal as selection criteria for employment. These requirements were formally gender-neutral but

6. CONCLUSION

This research exploits variation in the timing and outcomes of lawsuits brought against US state and local law enforcement agencies to estimate the long-term impact of temporary externally-imposed AA. We conducted a dynamic event study on the effects of being sued for discrimination, of operating under an externally imposed AA plan, and of emerging from such a plan.

We find that lawsuits alleging employment discrimination led to increased black police representation, with substantially larger gains when the litigation resulted in AA. Although AA increased black representation for all worker types, the gains were most dramatic for higher-ranked workers. The average cumulative impact of AA on the black representation gap in litigated departments was 4.5 percentage points for full-time and protective workers and 6.2 percentage points for professionals. There is no evidence that the substantial employment gains from temporary AA plans eroded following the termination of those plans. Changes in representation gaps after AA end dates were not significantly different from trends in un-litigated departments. However, they were significantly lower than trends for departments with ongoing AA.

The time pattern of the estimates, with sharp and significant changes in trend around litigation and termination dates, supports the interpretation of the estimates as causal effects of externally-imposed AA plans. To ensure that we controlled for the appropriate counterfactual time trends, we conducted several robustness checks, including region-year and state-year interactions and varying trends by litigation propensity. We considered and rejected several alternative explanations for the measured effects of AA starting and ending.

Female employment in police departments with externally-imposed AA plans increased by a larger absolute amount than the black representation gap. In the professional category, female employment shares increased by almost 30 percentage points in the 30 years following litigation, while black representation gaps increased by about 20 percentage points. However, when we controlled for national trends, the relationship was reversed. While the majority of the increase in black police employment during the sample period can be attributed to AA plans, the same is not true for the increase in female employment. For women, the gains in AA departments were also experienced by un-litigated departments. We conclude that Civil Rights enforcement and the use of court-ordered AA had a larger impact on employment by race than by sex.

This study is the first to quantify the long-term effects of the US experience of court-ordered AA in law enforcement. It can also be informative for decision-makers in other countries. Concerns regarding unrepresentative police forces are not particular to the US, and AA or “targeted recruiting” programs have

had an adverse impact on women. Banning them may have made it more difficult for departments to exclude women without overtly discriminatory practices.

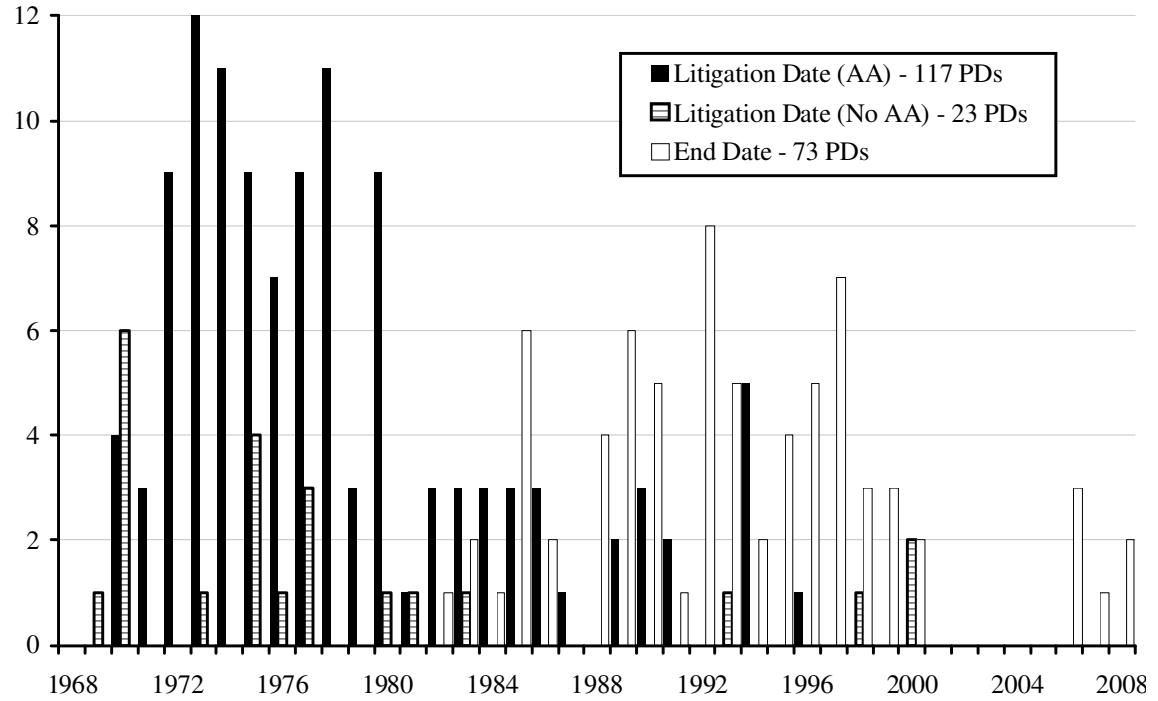
been debated and implemented in Europe as well (for example, see Independent Commission on Policing for Northern Ireland 1999). Although it is uncertain how effective employment-based AA would be in other settings, the results of this paper suggest that active court interventions, even of a temporary nature, can lead to lasting employment effects, especially with respect to racial disparities.

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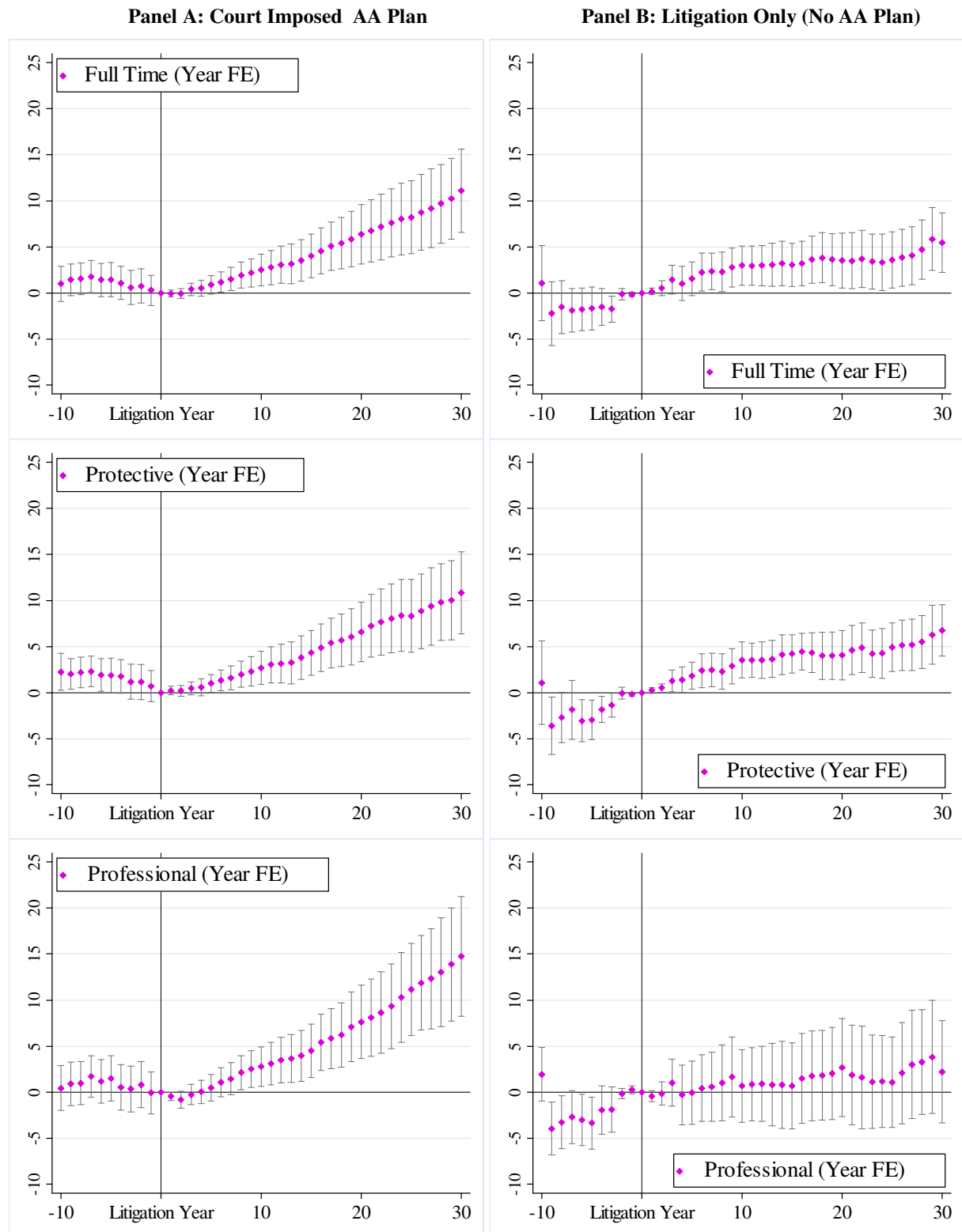
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Figure 1: Histogram of Litigation and Affirmative Action Dates



Source: Police AA case history database, compiled by authors. Details in the text.

Figure 2: Black Representation Gap Around Litigation Year by Litigation Result and Employment Category

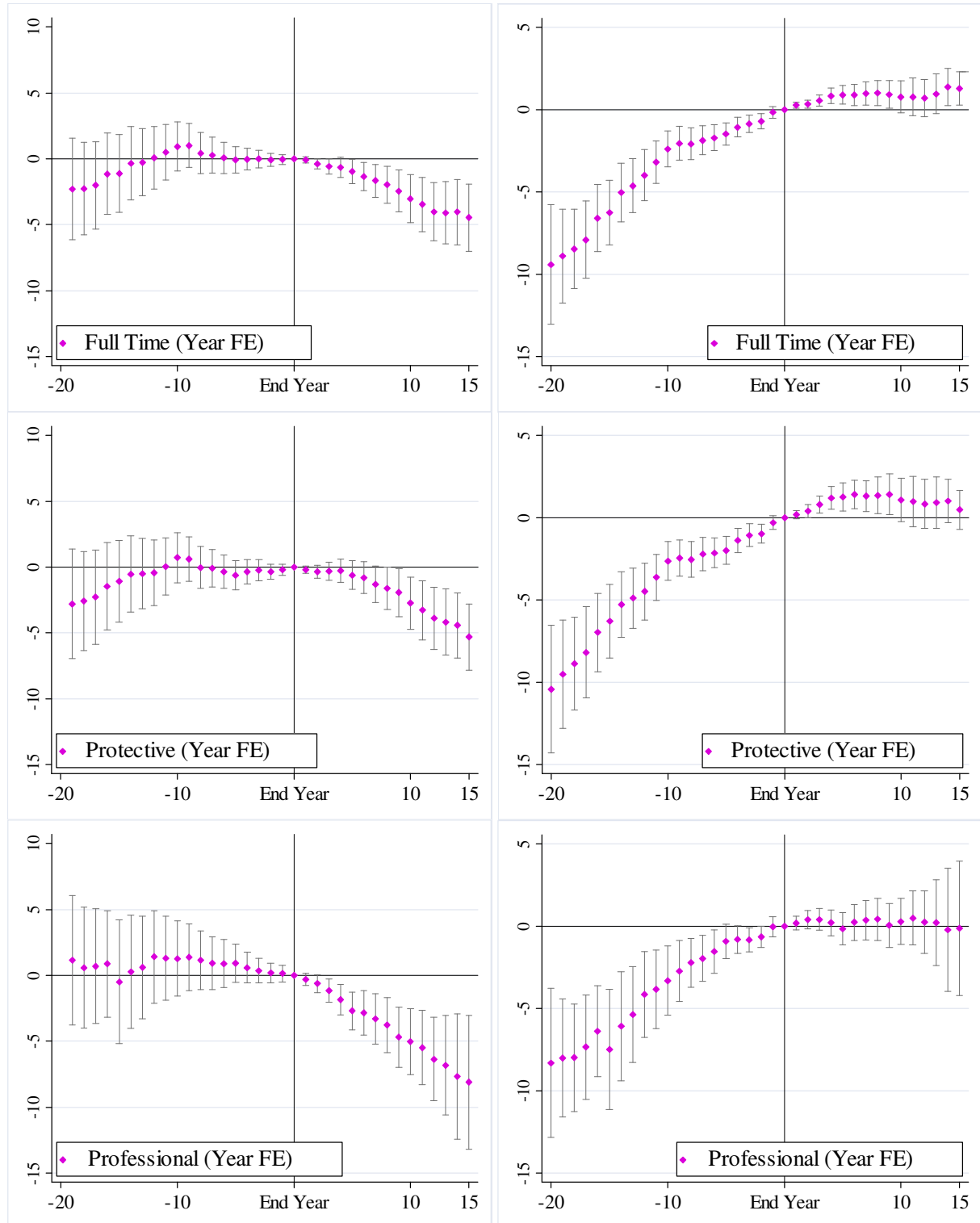


The figures show coefficient estimates and 90% confidence intervals on indicators for years before and after litigation: cases leading to AA in the left column, not leading to AA in the right. Model includes department and year effects.

Figure 3: Black Representation Gap Around End Year by Employment Category

Panel A: Relative to Depts. in which AA Continued

Panel B: Relative to Depts. that were Never Litigated

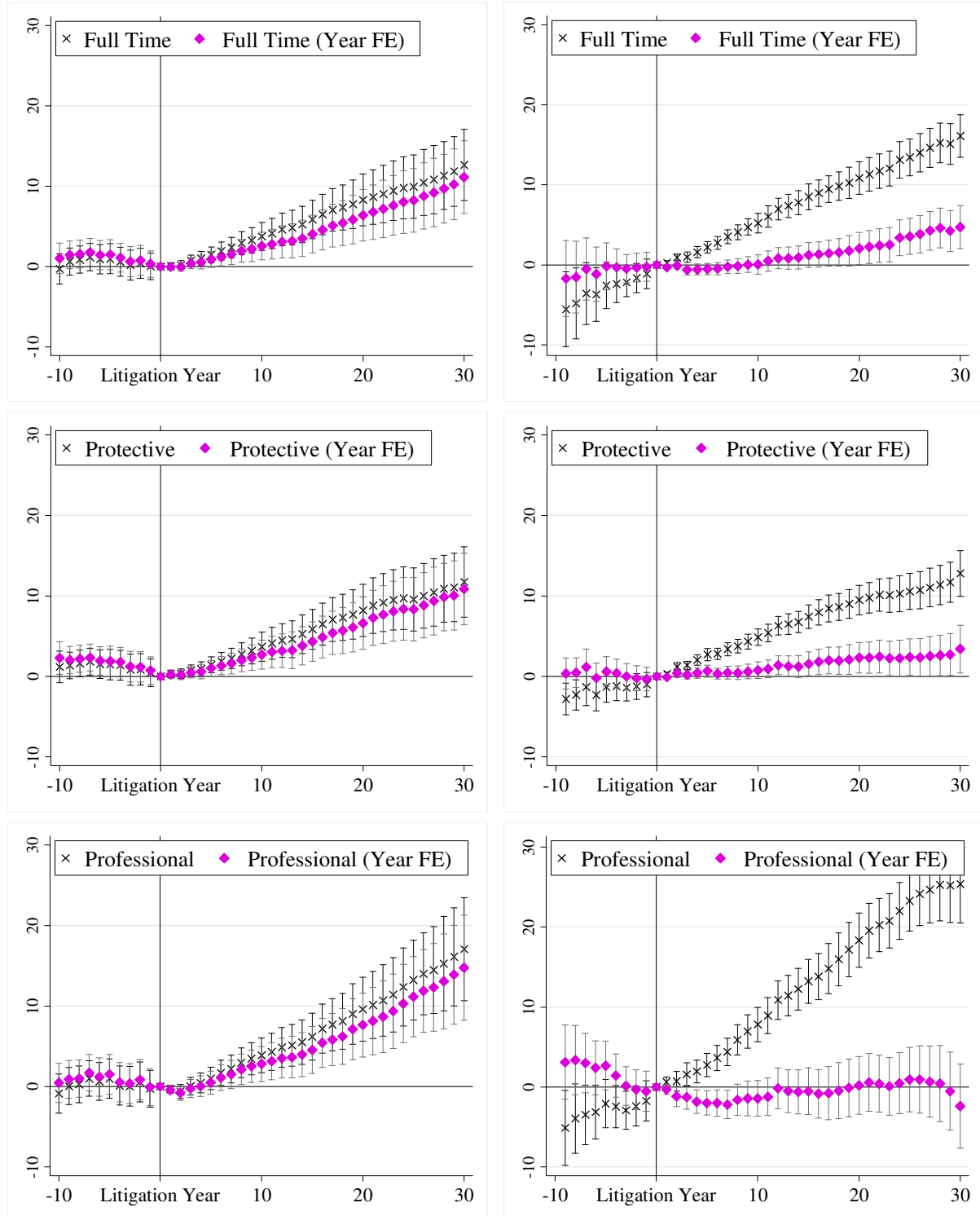


The figures show coefficient estimates and 90% confidence intervals on indicators for years before and after AA ending. Figures on the left are relative to AA, on the right relative to un-litigated. See text for details.

Figure 4: Effects of Court Imposed AA Plan on Blacks and Women by Employment Category

Panel A: Blacks

Panel B: Women



The figures show coefficient estimates and 90% confidence intervals on indicators for years before and after litigation leading to AA in models with (diamonds) and without (crosses) year effects.

Table 1: Means of Key Variables

	Never Litigated (No AA)		Litigated Only (No AA)		Court Imposed AA – No End Date		Court Imposed AA –End Date	
Number of Departments	339		23		44		73	
% in the South	38.3		39.1		45.5		47.9	
% in the Northeast	13.3		34.8		25.0		20.1	
% in the Midwest	19.2		17.4		20.5		17.8	
% in the West	29.2		8.6		9.0		13.7	
Mean Duration in Years of AA Plans (2005)					26.9		14.7	
Non-Missing Values for Full-Time Workers	1973	2005	1973	2005	1973	2005	1973	2005
Number of Departments	283	327	18	23	37	44	70	72
Number of Full-Time Employees	320.2	714.8	495	780	702.9	1044.3	2235.2	2949.2
% Black in Local Population	9.5	13.2	8.5	16.6	18.3	25.1	16.0	22.5
% Black in Full-Time Employment	4.5	10.3	5.5	15.9	6.9	24.1	5.8	19.7
Black Full-Time Representation Gap (%)	-5.0	-2.9	-3.1	-0.7	-11.3	-1.0	-10.3	-2.7
Black Protective Representation Gap (%)	-4.8	-3.4	-2.8	-0.5	-10.0	-2.2	-10.7	-3.5
Black Professional Representation Gap (%)	-7.3	-4.3	-5.9	-4.4	-15.1	-2.2	-12.9	-5.5
% Women in Full-Time Employment	14.8	28.6	12.6	23.7	12.3	29.8	12.7	28.1
% Women in Protective Employment	3.0	14.0	3.2	13.3	5.3	14.8	3.1	16.1
% Women in Professional Employment	4.7	33.1	4.6	19.7	5.3	32.5	4.8	34.1
Number with non-missing 1970 Census Variables	326		21		44		71	
Dissimilarity Index	0.77		0.79		0.77		0.77	
Isolation Index	0.49		0.53		0.55		0.57	
Total % Unemployed	3.8		3.7		3.5		3.7	
Black Male % Unemployed	6.2		5.5		5.6		6.0	
Total % Not in the Labor Force	28.4		27.6		28.5		29.0	
Black Male % Not in the Labor Force	10.9		10.5		10.3		11.3	
Total % High-School Graduates	69.1		69.7		66.9		66.7	
Black Male % High-School Graduates	48.2		49.6		45.5		45.5	
Total % Some College	30.5		30.4		28.3		28.2	
Black Male % Some College	16.6		17.8		14.0		15.3	

Sources: Police employment data are from EEO-4 reported for the years 1973-2005. Affirmative action and litigation information are from the Case History Database, compiled by the authors, described in Section 2.2 of the text. Black population shares are from CDC Wonder. Census employment and education information are from IPUMS. Measures of residential segregation are from Cutler, Glaeser and Vigdor (1999).

Table 2: Basic Parametric Model of the Effects of Litigation and AA on the Black Representation Gap

	Full-Time		Protective		Professional	
	1	2	3	4	5	6
Year Fixed Effects?	N	Y	N	Y	N	Y
Years Before Police Litigation (AA)	-0.0360 [0.072]	-0.182 [0.081]**	-0.092 [0.074]	-0.218 [0.082]***	-0.126 [0.087]	-0.236 [0.092]**
Years After Police Litigation (AA)	0.452 [0.067]***	0.370 [0.068]***	0.453 [0.068]***	0.387 [0.069]***	0.537 [0.084]***	0.446 [0.086]***
Years After Police AA End	-0.353 [0.108]***	-0.279 [0.108]***	-0.363 [0.109]***	-0.284 [0.109]***	-0.510 [0.182]***	-0.484 [0.183]***
Years Before Police Litigation (No AA)	-0.025 [0.194]	-0.118 [0.190]	-0.067 [0.204]	-0.143 [0.200]	-0.101 [0.122]	-0.195 [0.121]
Years After Police Litigation (No AA)	0.218 [0.060]***	0.151 [0.062]**	0.242 [0.055]***	0.193 [0.057]***	0.185 [0.100]*	0.099 [0.101]
Constant	-4.885 [0.173]***	-6.774 [0.319]***	-5.211 [0.175]***	-6.607 [0.335]***	-7.833 [0.220]***	-9.349 [0.412]***
Prob>F: YearsBeforeLit (AA) = YearsAfterLit (AA)	0.00	0.00	0.00	0.00	0.00	0.00
Prob>F: YearsAfterLit (AA) + YearsAfterEnd = 0	0.13	0.17	0.17	0.13	0.84	0.78
Prob>F: YearsAfterLit (AA) = YearsAfterLit (No AA)	0.01	0.01	0.02	0.03	0.01	0.01
Observations	15311	15311	15279	15279	15260	15260
R²	0.81	0.82	0.78	0.79	0.76	0.77

Notes:

1. All models include a full set of police department fixed effects.
2. Robust standard errors clustered by police department in brackets.
3. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Parametric Model of the Black Representation Gap with Geographically Varying Time Trends

	Full-Time		Protective		Professional	
	1	2	3	4	5	6
Year × Region Fixed Effects?	Y	N	Y	N	Y	N
Year × State Fixed Effects?	N	Y	N	Y	N	Y
Years Before Police Litigation (AA)	-0.174 [0.068]***	-0.189 [0.072]***	-0.202 [0.066]***	-0.247 [0.071]***	-0.247 [0.083]***	-0.24 [0.095]**
Years After Police Litigation (AA)	0.343 [0.061]***	0.320 [0.057]***	0.356 [0.063]***	0.331 [0.058]***	0.442 [0.076]***	0.423 [0.079]***
Years After Police AA End	-0.304 [0.098]***	-0.282 [0.102]***	-0.317 [0.102]***	-0.293 [0.113]***	-0.524 [0.173]***	-0.487 [0.182]***
Years Before Police Litigation (No AA)	-0.143 [0.172]	-0.136 [0.179]	-0.170 [0.196]	-0.19 [0.224]	-0.261 [0.116]**	-0.305 [0.139]**
Years After Police Litigation (No AA)	0.182 [0.066]***	0.195 [0.072]***	0.205 [0.061]***	0.206 [0.067]***	0.151 [0.106]	0.206 [0.114]*
Constant	-6.797 [0.290]***	-6.849 [0.285]***	-6.616 [0.316]***	-6.748 [0.313]***	-9.378 [0.376]***	-9.438 [0.375]***
Prob>F: YearsBeforeLit (AA) = YearsAfterLit (AA)	0.00	0.00	0.00	0.00	0.00	0.00
Prob>F: YearsAfterLit (AA) + YearsAfterEnd = 0	0.54	0.62	0.57	0.67	0.55	0.66
Prob>F: YearsAfterLit (AA) = YearsAfterLit (No AA)	0.06	0.16	0.07	0.14	0.02	0.10
Observations	15311	15311	15279	15279	15260	15260
R²	0.84	0.85	0.81	0.82	0.79	0.81

Notes:

1. All models include a full set of police department fixed effects.
2. Robust standard errors clustered by police department in brackets.
3. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Parametric Model of the Black Representation Gap Estimated on Litigated Departments Only

	Full-Time			Protective			Professional		
	1	2	3	4	5	6	7	8	9
Year Fixed Effects?	N	Y	N	N	Y	N	N	Y	N
Year \times Region Fixed Effects?	N	N	Y	N	N	Y	N	N	Y
Years Before Police Litigation (AA)	-0.031 [0.071]	-0.101 [0.182]	-0.002 [0.158]	-0.087 [0.073]	-0.299 [0.194]	-0.237 [0.201]	-0.117 [0.085]	-0.281 [0.244]	-0.109 [0.220]
Years After Police Litigation (AA)	0.450 [0.066]***	0.490 [0.177]***	0.578 [0.166]***	0.45 [0.067]***	0.349 [0.207]*	0.382 [0.211]*	0.533 [0.084]***	0.418 [0.253]*	0.585 [0.264]**
Years After Police AA End	-0.351 [0.108]***	-0.222 [0.114]*	-0.320 [0.108]***	-0.361 [0.109]***	-0.213 [0.112]*	-0.305 [0.114]***	-0.508 [0.182]***	-0.467 [0.189]**	-0.587 [0.176]***
Years Before Police Litigation (No AA)	-0.025 [0.194]	-0.005 [0.254]	0.09 [0.218]	-0.067 [0.204]	-0.188 [0.279]	-0.133 [0.267]	-0.101 [0.122]	-0.224 [0.267]	-0.15 [0.249]
Years After Police Litigation (No AA)	0.218 [0.060]***	0.285 [0.183]	0.388 [0.179]**	0.242 [0.055]***	0.172 [0.207]	0.218 [0.217]	0.185 [0.100]*	0.077 [0.260]	0.277 [0.267]
Constant	-8.784 [0.576]***	-10.407 [1.171]***	-10.001 [0.995]***	-9.42 [0.582]***	-11.162 [1.272]***	-10.948 [1.255]***	-13.154 [0.731]***	-14.481 [1.618]***	-13.758 [1.323]***
Prob>F: YearsBeforeLit (AA) = YearsAfterLit (AA)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Prob>F: YearsAfterLit (AA) + YearsAfterEnd = 0	0.13	0.17	0.17	0.18	0.54	0.74	0.85	0.86	1.00
Prob>F: YearsAfterLit (AA) = YearsAfterLit (No AA)	0.01	0.02	0.04	0.02	0.04	0.06	0.01	0.01	0.02
Observations	4544	4544	4544	4538	4538	4538	4543	4543	4543
R²	0.82	0.83	0.87	0.79	0.8	0.84	0.76	0.76	0.81

Notes:

1. All models include a full set of police department fixed effects.
2. Robust standard errors clustered by police department in brackets.
3. * significant at 10%; ** significant at 5%; *** significant at 1%
4. Sample restricted to litigated police departments.

Table 5: Robustness to Alternative Hypotheses: Parametric Models of Black Representation Gaps with Additional Controls

Alternative Hypothesis	Rep. Gap Levels Off at Some Point			Longer Time Horizon for Erosion of Gains		
	1 Full-Time	2 Protective	3 Professional	4 Full-Time	5 Protective	6 Professional
Year \times Region Fixed Effects?	Y	Y	Y	Y	Y	Y
Years Before Police Litigation (AA)	-0.174 [0.066]***	-0.196 [0.065]***	-0.243 [0.083]***	-0.179 [0.068]***	-0.197 [0.066]***	-0.247 [0.083]***
Years After Police Litigation (AA)	0.343 [0.060]***	0.356 [0.061]***	0.441 [0.075]***	0.34 [0.060]***	0.352 [0.062]***	0.435 [0.076]***
Years After Police AA End	-0.319 [0.119]***	-0.448 [0.137]***	-0.408 [0.169]**	-0.338 [0.102]***	-0.414 [0.112]***	-0.468 [0.154]***
High Representation Gap \times Years After Police AA End ⁴	0.033 [0.112]	0.235 [0.126]*	-0.231 [0.251]			
No Significant Growth in Dept. Size \times Years After Police AA End				0.223 [0.158]	0.263 [0.120]**	-0.409 [0.397]
Significant Negative Growth in Dept. Size \times Years After Police AA End				0.13 [0.161]	0.574 [0.265]**	0.282 [0.267]
Years Before Police Litigation (No AA)	-0.144 [0.169]	-0.171 [0.192]	-0.258 [0.115]**	-0.143 [0.171]	-0.169 [0.195]	-0.257 [0.117]**
Years After Police Litigation (No AA)	0.182 [0.065]***	0.209 [0.060]***	0.148 [0.104]	0.181 [0.066]***	0.203 [0.061]***	0.148 [0.106]
Constant	-6.798 [0.285]***	-6.609 [0.309]***	-9.372 [0.371]***	-6.803 [0.289]***	-6.608 [0.315]***	-9.38 [0.377]***
Observations	15311	15279	15260	15311	15279	15260
R²	0.84	0.81	0.79	0.84	0.81	0.79

Notes:

1. All models include a full set of police department fixed effects.
2. Robust standard errors clustered by police department in brackets.
3. * significant at 10%; ** significant at 5%; *** significant at 1%
4. High Representation Gap departments are those with representation gaps above the median for departments with ongoing AA in the calendar year in which AA ends. In the first column the median is defined with respect to full-time employment, in the second, protective employment, and in the third, to professional employment.

Table 6: Parametric Model of the Black Representation Gap with Controls for School Desegregation

	Full-Time		Protective		Professional	
	1	2	3	4	5	6
Year Fixed Effects?	Y	N	Y	N	Y	N
Year \times Region Fixed Effects?	N	Y	N	Y	N	Y
Years Before Police Litigation (AA)	-0.165 [0.083]**	-0.152 [0.070]**	-0.204 [0.084]**	-0.183 [0.070]***	-0.223 [0.092]**	-0.234 [0.081]***
Years After Police Litigation (AA)	0.351 [0.067]***	0.322 [0.058]***	0.371 [0.068]***	0.338 [0.061]***	0.430 [0.084]***	0.430 [0.073]***
Years After Police AA End	-0.29 [0.108]***	-0.307 [0.099]***	-0.295 [0.110]***	-0.321 [0.104]***	-0.493 [0.182]***	-0.526 [0.173]***
Years Before Police Litigation (No AA)	-0.114 [0.182]	-0.140 [0.163]	-0.140 [0.192]	-0.168 [0.187]	-0.190 [0.110]*	-0.258 [0.108]**
Years After Police Litigation (No AA)	0.115 [0.062]*	0.148 [0.069]**	0.160 [0.059]***	0.174 [0.064]***	0.066 [0.095]	0.129 [0.102]
Years Before School Desegregation Start	-0.149 [0.198]	-0.01 [0.192]	-0.150 [0.182]	-0.028 [0.182]	-0.199 [0.202]	-0.095 [0.207]
Years After School Desegregation Start	0.073 [0.035]**	0.065 [0.034]*	0.068 [0.037]*	0.061 [0.038]	0.083 [0.043]*	0.060 [0.041]
Years Before School Desegregation End	0.115 [0.067]*	0.087 [0.067]	0.100 [0.071]	0.078 [0.072]	0.069 [0.077]	0.033 [0.074]
Years After School Desegregation End	-0.268 [0.149]*	-0.231 [0.151]	-0.341 [0.151]**	-0.306 [0.157]*	-0.315 [0.175]*	-0.301 [0.160]*
Constant	-6.468 [0.408]***	-6.477 [0.367]***	-6.37 [0.433]***	-6.356 [0.398]***	-9.263 [0.520]***	-9.336 [0.477]***
Prob>F: YearsBeforeLit (AA) = YearsAfterLit (AA)	0.00	0.00	0.00	0.00	0.00	0.00
Prob>F: YearsAfterLit (AA) + YearsAfterEnd = 0	0.39	0.83	0.3	0.82	0.65	0.49
Prob>F: YearsAfterLit (AA) = YearsAfterLit (No AA)	0.01	0.05	0.02	0.05	0.00	0.02
Observations	15311	15311	15279	15279	15260	15260
R-squared	0.82	0.84	0.79	0.81	0.77	0.79

Notes: See Table 2.